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OBSERVATION AND THEORY IN BEHAVIOR ANALYSIS

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"Mental representations of visual objects play an important role in guiding everyday behavior" (Cooper & Schacter, 1992, p. 141). This quotation captures much of the tension between researchers in the experimental analysis of behavior and the contemporary surrounding cultural and scientific context. Many regular readers of this journal will feel that the quotation reveals misleading and counterproductive ideas. One is that mental representations that are not observable, and by some accounts may not even have physical instantiation, are nevertheless scientifically legitimate. Another is that such representations can and do cause behavior. Many readers will want to note that indelicate use of such ideas can cause logical, conceptual, epistemological, and ontological errors (Rorty, 1979; Ryle, 1949; Skinner, 1984; Wittgenstein, 1953).

There may be no way to do science, however, that guarantees freedom from error. My general purpose here is to suggest that the way in which behavior analysts try to avoid these particular errors may create some of the problems Staddon identifies in his present essay.

It is highly laudable, of course, for Staddon to be "more alarmed by legitimate criticisms of the experimental analysis of behavior than by the weaknesses of its competitors" (p. 439). It is also laudable that the editors of this journal encourage the expression of views such as Staddon's in its pages. His criticisms define exciting problems for behavior analysts to solve. I would like to focus my comments on two of these problems: the role of observation and the role of theory in behavior analysis. My specific goal is to suggest that observation and theory do not have to play the restricted roles commonly assigned to them in this approach.

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Observation in Behavior Analysis

To be sure, behavior analysts see some nonobservable phenomena as acceptable in principle, but no other research tradition views the practice of invoking them with more suspicion (e.g., see Zuriff, 1985). Sometimes these phenomena appropriately earn the epithet of "explanatory fiction." Staddon implies, however, that it is also possible to throw out some good science if we routinely throw out nonobservable phenomena. Consider a nonobservable phenomenon such as a memory, and compare its conceptual and empirical status to that of mean response rate. The issue I raise is whether there is a difference between memory and response rate, in terms of empirical, conceptual, or scientific legitimacy. An example I have described before in some detail (Shimp, 1976) involves the status of a representation of a higher order category label, such as *flower* for names of specific kinds of flowers, in a list of to-be-remembered words that might be presented to an experimental subject (Tulving, 1962). Let us suppose *flower* does not appear in the list but the specific names do. The experimenter does not then observe the category label, and certainly not its representation, but one does observe correct or incorrect recall of the specific names, and one can keep track of the order in which they are recalled. Based on these observations and records, one can do some easy calculating and come up with a number. This number, along with both the empirical procedures that generate it and ways of talking about it, define the representation of the category label (Tulving, 1962). The variable V in Staddon's cumulative effects (CE) model has the same status. Such a number is sometimes seen by behavior analysts to reflect a subversive sort of bad science.

This suspicious view may be problematic, because the origin of this number is remarkably similar to that of mean response rate, where one observes responses and does some simple calculating. In the case of "subjective organization in memory," behavior analysts prefer to ban the process and refer to explanatory fictions. In the case of mean response rate, behavior analysts typically prefer to refer to the process as the foundation of a science of behavior and to the result as a fundamental dimension of behavior. Clearly, something else besides pure empirical methodology or empirical data determines the legitimacy of variables within behavior analysis, for otherwise subjective organization and mean response rate would share equal honors. The nature of this "something else" leads us to the next topic.

Theory in Behavior Analysis

An important implication of Staddon's comments is that there is an interdependence between the meaning of observation, on one hand, and explanations of behavior in terms of the environment (external explanations) or the organism (internal explanations), on the other hand. Staddon's example of the CE model clearly demonstrates interdependence between theory and observation. Yet as Staddon describes it, this interdependent relation has sometimes gone unnoticed or has been dismissed as of little practical significance. For instance, Skinner (1984, p. 544) wrote "So much for the usefulness of theory" in a reply to a commentary (Shimp, 1984) on his paper (Skinner, 1984). Skinner's comment represents a common and legitimate point of view, but it has its drawbacks as well as its strengths. The nature of the "something else" referred to in the previous paragraph is an assortment of only partly articulated priorities and assumptions, of conventional practice and traditions, and of linguistic customs imported from surrounding culture. This guiding framework is part of the "theory laden" characteristic of much good science. It is also roughly what Staddon refers to as conventional wisdom.

It is not meant as a criticism of behavior analysis that the scientific community that practices it is influenced by its surrounding cultural context. All scientific practice may inescapably depend on its cultural and historical context (Geertz, 1973; Hanson, 1958). Consider the development of behavior analysis. It may not be coincidental that a scientific research community studying the behavior of individual organisms arose in an American culture that praises individual freedom and liberty, "rugged individualism," and so on. In addition, it has not gone unnoticed that early behaviorism may have been particularly appealing to an American public in favor of egalitarianism, a public that was glad to see the possibility of ensuring equal opportunity through individuals' interactions with their environments. And it is scarcely imaginable, of course, how today's "basic" schedules of reinforcement could have evolved without the electromechanical technology of the 1930s. It is difficult to imagine, in other words, that a behavioral scientist handed a modern high-resolution engineering work station instead of a handful of relays and timers would develop these same "basic" contingencies. Many other similar examples of cultural influences on the development of behavior analysis could be provided. My point is, I believe, similar to Staddon's. We must be careful not to assume either that our notion about observation or our values about established methodology are somehow so privileged, so purely scientific and objective, that we fail to struggle to uncover, make explicit, and evaluate arbitrary elements in our conventional wisdom.

Consider the idea that a good theory in behavior analysis is one at the same level as behavior. Note that this part of conventional wisdom interrelates observation and theory. Clearly, this conventional wisdom would be of less use if we were uncertain about what behavior was, or how it was different from non-behavior, whatever that could be. In order to make sense of these distinctions we should known what the dimensions of behavior are. If one accepts the assumption, as Skinner did, that mean response rate is a "fundamental datum," then one has a tool with which to discriminate between theories on the same or different level as behavior. But if one does not accept this assumption, then the most common evaluative criterion for theory in behavior analysis disappears. Several of Staddon's recommendations seem designed to deal with the consequence of this disappearance. He recommends the development of a more dynamic approach in terms of which long-term mean response rate is of less critical significance, and he recommends theoretical models that do not depend on the assumption that mean response rate is a fundamental datum. These recommendations seem likely to facilitate exciting experimental and theoretical innovations in behavior analysis.

Thus, some of the heat in dialogues involving molar and molecular analyses has its origin in the importance of the conceptual status of mean response rate to the establishment of a fundamental dimension of behavior, without which we do not know, in a rigorous scientific sense, what behavior is. A molecular conceptual and experimental deconstruction of mean response rate seems to be at risk in a behavioral community in which behavior, to an important practical degree, *means* response rate. Similarly, a molar analysis dependent on response rate might be threatened by a molecular analysis deriving average rate from more nearly fundamental local and diverse patterns of behavior. Fortunately for the sake of the constructive dialogue between molar and molecular approaches, it is becoming increasingly clear that each approach can benefit from the other. For example, the amazing success of molar analyses over the last few decades has produced elegant quantitative descriptions (Davison & McCarthy, 1988; Williams, 1988) that place the kinds of constraints on dynamic and molecular models without which such models scarcely could be developed at all. These dynamic models, two of which Staddon describes, offer the possibility of generating the classic molar phenomena and thus linking local, molecular dynamics with large-scale, molar averages. Some of these dynamic theories actually "behave," in the sense that they can generate real-time response protocols (Kehoe, 1989; Shimp, 1989; Staddon & Bueno, 1991; Wearden & Clark, 1988). As such, these models appear to fit Staddon's recommendations remarkably well. Because the real-time behavioral processes continuously interact, at no time through simple observation of behavior can one see or observe any of the individual processes. What one observes are the emergent properties of their interactions (for details, see Shimp, 1992; Shimp & Friedrich, 1993). These theories cast an interesting light on the nature of behavior. Instead of behavior defined in terms of cumulative records or response rates, one finds behavior defined in terms of behavior processes, the existence of which we can infer but not observe. Observed behavior is an emergent property of the real-time interactions of these behavioral processes. This sacrifice of direct observation appears to be a reasonable trade-off in order to achieve the development of theories that, like real organisms, behave in real time.

The Uniqueness of Behavior Analysis

As noted above, the evolution of behavior analysis depends on its surrounding cultural and technological context. The heavy and growing dependence on computer technology presumably will lead to the development of more computational models to permit the description of rich details of real-time individual-behavior protocols. These models invite a reexamination, as Staddon suggests they should, of the relation between observation and theory, because they may involve behavior processes that are not directly observable but that are indirectly identifiable. Powerful computer models may capture naturalistic detail of individual behavior and may promote an integration of machine models and more ethnographic, descriptive, and naturalistic analyses encouraged by an increasingly dominant contextualist philosophy of science (Geertz, 1973; Hanson, 1958). As Staddon's examples show, one context that will play a more explicit theoretical role is historical context in the form of a detailed reinforcement history.

Staddon surely is correct in that behavior analysis will change. One could therefore ask what developments outside of behavior analysis, in, say, contemporary cognitive psychology, might potentially play a legitimate role in this change. A review of the history of cognitive science (Gardner, 1985) showed that virtually all of the exciting research problems in that new field can be studied, in fact already are being studied, with the tools of behavior analysis (Shimp, 1989). This kind of assimilation by behavior analysis of new topics from outside it helps to sustain its energy and vitality.

Does this importation of new ideas mean that behavior analysis will merge with the rest of psychology and lose its distinctiveness? If it were defined in terms of a particular philosophical position on the nature of observation, then yes, it would. If it were defined in terms of a particular position on the role of theory in science then the answer would, again, be yes. However, the traditional positions within behavior analysis on these issues are not central to its identity. A good theory in behavior analysis does not necessarily have to be at the level of behavior (even if we knew what that meant). A good theory does not necessarily have to be arrived at inductively. A good theory does not have to refer only to observables. Similarly, behavior analysis does not have to rest on, or in principle even have to involve, mean response rate.

What then preserves the identity of behavior analysis? The defining characteristic is in analysis of behavior of *individual* organisms (see the masthead of this journal). Behavior analysis is predicated on the assumption that there is something special, and in a sense privileged, about a science that concentrates on individual organisms. It is privileged because it produces methods and explanations that are usefully applicable to everyday behavior as well as to its own development as a science. It is even possible that the quotation with which this article begins could at some time in the future have a legitimate interpretation and useful function within the experimental analysis of behavior. Staddon's recommendations to focus more on a dynamic-systems approach that explicitly involves reinforcement history and computational theory seems likely to facilitate the development of an experimental analysis of behavior that can deal with complex everyday behavior.

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